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# Investment Decisions Depend on Portfolio Disclosures

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# Investment Decisions Depend on Portfolio Disclosures

## **Abstract**

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## **Disciplines**

Finance | Finance and Financial Management

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Financial  
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*Investment Decisions Depend  
on Portfolio Disclosures*

by  
David K. Musto

97-49

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# Investment Decisions Depend on Portfolio Disclosures <sup>1</sup>

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**Abstract:** A weekly database of retail money fund portfolio statistics is uneconomical for retail investors to observe, so it allows direct comparison of disclosed and undisclosed portfolios. This allows for a more direct and unambiguous test for “window dressing” than elsewhere in the literature. The analysis shows that funds allocating between government and private issues hold more in government issues around disclosures than at other times, consistent with the theory that intermediaries prefer to disclose safer portfolios.

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A money manager is typically required to disclose his portfolio on pre-arranged dates. What can we learn from these disclosures? It is up to the manager. He could ignore his disclosure schedule when he invests, in which case the disclosed portfolios would be unbiased, albeit noisy, estimates of his investing strategy. But he could instead plan his investing around the schedule by moving briefly in and out of the portfolio he prefers to disclose, thereby reducing or destroying the power of his disclosures to reveal anything meaningful. The goal of this paper is to establish which scenario applies to money market mutual funds.

The idea that portfolio weights interact with disclosure dates surfaces now and again in the academic and trade literatures, surviving on circumstantial evidence rather than direct observation. The odd behavior of asset prices around common disclosure dates, particularly the end of the year, comprises most of this evidence. Haugen and Lakonishok (1988) argue that riskier stocks and bonds and recent losers are cheap at year-end because disclosing intermediaries avoid them, a strategy they term *window dressing*, and Musto (1997) makes the same case for year-end and quarter-end price shifts in the money market. But the incidence of window dressing remains an open question, reflecting the difficulty of detecting what managers have putatively tried to conceal. The empirical tests below address this problem by analyzing a database of retail money funds which is uneconomical, though not impossible, for retail investors to observe. This allows us to observe portfolio adjustments that were effectively concealed from the funds' clientele.

The data in question are the portfolio statistics collected weekly by the newsletter *Money Fund Report*. A substantial majority of money funds report several current statistics, including the allocation between government and private issues, through this publication. The power of the data to detect window dressing follows from three facts: 1)

private issues are generally regarded as riskier than government issues, 2) The government/private allocation is disclosed to the general public only twice a year, and 3) at \$1,195/year, the newsletter is uneconomical for almost all retail investors to observe. A strategy of holding more in lower-risk government securities around disclosures than before or after would be effectively unobservable to the retail clientele but easily detectable in the data.

The rest of the paper is in four sections. Section I covers the window dressing literature and some special considerations that apply to the money market, section II describes the money fund database and the associated database of disclosure schedules, section III designs and executes the empirical tests, and section IV summarizes and concludes.

## I. Background and Literature

It is well known that many portfolio managers have the opportunity to window dress. Under the Investment Company Act of 1940, a mutual fund manager has to disclose his portfolio in complete detail as of his fiscal year-end and six months later, but has no obligation to reveal positions held on other dates of the year. A manager could in principle disclose a portfolio completely different from the one he usually holds. For an equity manager this would involve transactions costs around two times the average bid/ask spread, but for a money fund manager the transactions costs could be very low, or even zero. Money market instruments mature quickly, and the proceeds must be reinvested somewhere, so there isn't necessarily a transactions cost to tilting from one sort of portfolio to another. So money managers, and especially money fund managers, can easily window dress if they want to.

Whether fund managers want to window dress turns on what they believe outsiders infer from disclosures. There is plenty of evidence that outsiders infer *something* from disclosures. Consumers pay for services like *Morningstar* which report and analyze disclosed portfolios, and academics often use disclosed portfolios as proxies for undisclosed portfolios (e.g. Chevalier and Ellison (1995), Sias and Starks (1997)). If managers conclude that disclosures influence judgments about their professional behavior or ability, it is at least plausible that they would want to window dress.

This motive is stronger when money funds are involved because there is little information outside of portfolio disclosures about a money fund's risk. In the standard performance-measurement paradigm, there are three main sources of variation across managed funds' returns: skill; noise; and risk profiles. Thus, investors trying to identify skilled managers by their realized returns are advised to net out their risk profiles first.



Starting with Jensen (1969), the finance literature has developed performance measures which estimate risk profiles from regressions of returns on factors, without reference to the underlying portfolios. These measures are now staples of the fund profiles sold to investors. But money funds are different in a way that makes them relatively uninformative.

Money funds follow a peculiar accounting standard known as the *amortized cost method*, in which the reported value of a security under sixty days to maturity follows a straight-line amortization. For example, if a fund buys \$1M face value of 30-day commercial paper (CP) for \$994,000, it will value the paper at \$996,000 in ten days and \$998,000 in twenty days, regardless of developments in interest rates and risk premia. The result is that the realized returns of a money fund are relatively uninformative about the risks it is exposed to. Suppose fund *A* holds only thirty-day CP with a discount rate of 6%, and fund *B* holds only thirty-day Treasury bills, also at 6%. If *A* and *B* make no transactions for the next thirty days, and the CP that *A* bought does not default, their returns will be precisely the same. If the default-risk premium jumps up, the market value of fund *A* will drop, but its advertised net asset value and yield will be unaffected so an outsider would have no idea that *A* was riskier than *B*. Defaults are very rare, so this sort of confusion is typical. The low information content of money funds' returns makes their portfolio disclosures relatively more important, which in turn strengthens the motive to window dress.

Empirical research has focused not on whether window dressing occurs but on whether it moves prices at the year-end. Haugen and Lakonishok (1988) argue that window dressing, and not tax-loss selling, may be responsible for the big January returns of small stocks and recent losers known as the January Effect. That is, the affected stocks

go up after the year-end because institutions with year-end disclosures have stopped avoiding them, not because individuals have stopped selling them. Much of the evidence on window dressing comes from tests of this theory.

Some of the tests compare institutional and individual trading at the year-end. Dyl and Maberly (1992) find that odd-lot orders (i.e. not blocks of 100 shares), generally associated with individuals rather than institutions (e.g. Lakonishok and Maberly (1990)), are biased toward sales in the last days of the year. They conclude that at least some of the January Effect reflects tax-motivated selling and not window dressing. Ritter (1988) shows that individuals bias toward selling at year-end, and reaches the same conclusion. Sias and Starks (1997), looking at transactions data, find that trades by institutions were biased toward buying winners in the last days of 1990 (relative to the first days of 1991), which suggests window dressing. They do not, however, find any evidence of a bias toward selling losers, and their data do not indicate whether the institutions involved really had year-end disclosures.

Some other tests analyze patterns in disclosed portfolios. Lakonishok, Shleifer, Thaler and Vishny (1991) (LSTV) study quarter-end portfolio reports by pension fund managers. These portfolios are all disclosed in the sense that the typical pension plan administrator can view his managers' portfolios at any point, not just year-ends or quarter-ends. As such, they can not reveal anything about how disclosed portfolios compare with undisclosed portfolios. But LSTV contend that administrators may pay more attention to year-end portfolios in performance evaluations, and that this would induce managers to window dress during the fourth quarter. LSTV find that both sales and purchases of recent losers are relatively high in the fourth quarter, and they take the sales as evidence of window dressing.

The database of Sias and Starks (1997) is similar in that it comes from public filings by institutional investors. For each stock, they have the aggregate investment by institutions, as reported in recent disclosures. They reason that institutions are relatively inactive in the stocks showing low aggregate investment, so when they find that these stocks have relatively larger January Effects, they conclude that the January Effect does not reflect institutional activity such as window dressing. But their result could as easily be read the other way, because the stocks subject to window dressing should *per force* be under-represented in disclosures. There may be no way to resolve this ambiguity by studying public disclosures, but there are still some ways to gauge what intermediaries do differently when outsiders aren't looking.

Musto (1997) shows that money market prices have something like a January Effect – big upward shifts for CP across the year-end, bigger shifts for riskier CP, no shifts for Treasury bills – and tests for window dressing on the Federal Reserve's quarter-end aggregate industry portfolios. These data allow some comparison between disclosed and undisclosed portfolios, because in some cases they reflect portfolios not disclosed individually to the public. For example, life-insurers do not reveal their quarter-end money-market holdings to claimholders or regulators, but they do reveal them to the American Council of Life Insurers, which reports aggregates by instrument type to the Fed. Every life insurer has to disclose its December 31 portfolio, so estimating the effect of disclosures on CP holdings is simply a matter of comparing the year-end and quarter-end figures. Tests of this sort show the significant negative relationship between year-end CP investment and industry-wide disclosure events consistent with window dressing. They can not, however, confirm that individual intermediaries are window dressing; the patterns in the aggregate data could in principle represent flows between institutions,

rather than flows within the institutions' portfolios.

These papers have improved understanding of year-end price shifts and aggregate effects of institutional trading, but they also demonstrate the need for a direct comparison of the disclosed and undisclosed portfolios of individual intermediaries. This sort of comparison may seem infeasible, but *Money Fund Report* makes it possible. Because the subscription price of \$1,195/year is uneconomical for almost all retail investors<sup>i</sup>, its weekly statistics on retail funds are effectively undisclosed. These statistics are not complete risk profiles of the funds, but they do track default risk by showing the current allocation between government and private-issue securities. Testing for window dressing is therefore a simple matter of identifying the weeks proximate to semiannual disclosures, and then comparing the statistics from these weeks with the statistics from the other weeks. The data on institutional funds are useful as a comparison.<sup>ii</sup> The subscription price is much more economical for institutions with millions of dollars to allocate, so the weekly statistics on institutional funds are *not* effectively undisclosed. Tests for window dressing should detect it in the first sample and not the second.

### **III. The Data**

The tests below combine two databases on taxable money funds, one with weekly statistics on their portfolios, the other covering their fiscal year-ends (FYE). Every week, a manager of a money fund can report several statistics on his portfolio to *Money Fund Report* (MFR). These include the net asset value, recent 7-day and 30-day yields and average maturity of portfolio holdings, each of which is reported in many newspapers, and they also include the percent allocation to Treasury securities, other government issues, commercial paper, bankers' acceptances, and other money market categories, which are

*not* reported in newspapers. Reports to MFR are voluntary, and the publishers estimate that they cover 95% by number and 98% by value of all money funds. Newsletters are dated Friday, but the portfolio statistics they contain refer to the previous Tuesday, which is the date we will use here. The database covers the 504 consecutive Tuesdays from November 24, 1987 to July 15, 1997.

MFR sorts taxable funds by whether they are retail or institutional, and by whether they are government-only or unconstrained.<sup>iii</sup> Institutional funds market to other institutional investors, and have high minimum balances. A million dollars is typical. All other funds are considered retail, though some retail funds have minimum balances that make sense for only a few individuals (e.g. \$100,000). Government-only funds restrict themselves to some combination of Treasury issues, Agency issues, and repos of Treasury issues. All other funds are considered unconstrained. Funds could and occasionally did change their types over the sample period. The tests below focus exclusively on the unconstrained funds, which numbered 435 as of July 15, 1997: 280 retail funds managing \$418,424 million, and 155 institutional funds managing \$172,691 million.

A useful feature of the MFR data is that there is no look-ahead bias; data for one week is not retroactively altered or removed in response to events of later weeks. It includes every report by every fund for every week, regardless of whether the fund later folded, merged, or stopped reporting. Missing observations are very rare; of the 150,768 reports by unconstrained funds (36,089 by institutional funds and 114,779 by retail funds), only 52 do not indicate the allocation to government securities, and there are only 60 missing reports by unconstrained funds.<sup>iv</sup> 629 different unconstrained funds are included for at least one week.

The data on FYEs have several sources. The main one is the library of SEC filings

on LEXIS/NEXIS, which identifies the FYE of almost every fund that operated in the last three years of the sample period. For funds that ceased operating before this period, FYEs were identified with data from Lipper Analytical Services, *Investment Dealer's Digest*, and the 1987-91 volumes of *Investment Companies* from Wiesenberger Financial Services. These sources yield FYE data on 573 of the 629 unconstrained funds, including 406 of the 452 funds that are retail funds in at least one week and 205 of the 220 funds that are institutional in at least one week.<sup>v</sup> All of the 56 funds without FYE data no longer report to MFR, and many have dissolved. This biases the usable MFR data a little toward surviving funds.

Following the Investment Company Act of 1940, we assume that each fund discloses the portfolio it holds on its FYE, and also the portfolio it holds six months later. A few funds elect to disclose quarterly; for these funds our procedure will misclassify some disclosure dates as non-disclosure dates. To give a sense of the popular reporting schedules, Table I shows the distribution of fiscal year-ends and the associated assets under management for the unconstrained retail and institutional funds in the July 15, 1997 issue. The numbers vary considerably across months, and by far the most popular schedule for retail funds is June 30/December 31, accounting for 39% of the total by number and 37% by assets under management.

### **III. Tests for Window Dressing**

Unconstrained funds ("funds," from here on) allocate throughout the year between government and private-issue securities. Table II collects a few summary statistics on these allocations (calculated for each fund individually and then averaged across funds), showing a mean around 8% and substantial time-series variation within both retail and

institutional funds. The tests of this section are designed to establish whether some of the time-series variation within retail funds reflects the disclosure schedule: higher during semiannual disclosure dates than in the rest of the year, as the window-dressing hypothesis predicts. For comparison, the tests are repeated on institutional funds, for which the window-dressing hypothesis has no prediction.

#### *A. Are Government Allocations Bigger During Disclosures?*

A fund has two disclosure dates in a year, the ends of its second and fourth fiscal quarters. The empirical question is how its government investment on these dates compares with its government investment in the rest of the year. This comparison is very simple but it has to take into account that the MFR data cover only Tuesdays. Let  $T(d)$  be the Tuesday closest to date  $d$ , and let the Tuesdays of year  $y$  start after  $T(12/31/y-1)$  and end with  $T(12/31/y)$ . So for example,  $T(12/31/89) = 1/2/90$  and  $T(12/31/90) = 1/1/91$ , so the Tuesdays of 1990 run from 1/9/90 to 1/1/91. A fund's government allocation on a non-Tuesday disclosure date must be estimated from disclosures on nearby Tuesdays. My estimate is the allocation reported for the Tuesday closest to the disclosure date, rather than the last Tuesday on or before it or the first Tuesday on or after it, since disclosure-related allocations are as likely to show up just before the disclosure date as just after.<sup>vi</sup>

The estimate of fund  $f$ 's disclosed government allocation in year  $y$  is denoted  $DISC_{f,y}$  and defined as the average over the two Tuesdays closest to  $f$ 's two disclosure dates in  $y$ . The estimate of  $f$ 's undisclosed allocation in  $y$ ,  $UNDISC_{f,y}$ , is the average over all other Tuesdays of  $y$ . Consider Scudder Cash Investment Trust (SCIT), which has a June fiscal year. The value of  $DISC$  is 7.0 for 1990 because its government allocation was 2% on  $T(6/30/90)=7/3/90$  and 12% on  $T(12/31/90)=1/1/91$ , and  $UNDISC$  is 1.56, the

average over the 50 other Tuesdays of 1990. For every year  $y$  from 1988 to 1996 and for every fund  $f$  with data for all of  $y$  I let  $Z_{f,y} = DISC_{f,y} - UNDISC_{f,y}$ , and test across all years and retail funds whether  $Z$  is significantly positive. I then repeat the test on the sample of institutional funds. The results are in Table III.

The test shows that the retail funds do in fact allocate more to government securities during disclosures. The point estimate is 0.296% of the portfolio on average, which is about 4% of the average undisclosed allocation to government securities (i.e.  $0.296/7.678 \approx 0.04$ ), and is significantly positive at the usual levels. To put this number in perspective, 0.296% of the \$418 billion managed by retail funds is about \$1 billion. One interpretation is that someone who took disclosed portfolios to be unbiased estimates of undisclosed portfolios would have overestimated the undisclosed allocation to government securities by this much.

The test statistic is no longer significant, and the point estimate less than half as big, when retail funds are replaced by institutional funds. This pattern supports the hypothesis that window dressing drives the reallocations detected in retail funds.

#### *B. Do Investment Decisions Depend on Disclosure Events?*

The results of Table III do not go all the way to showing that disclosure events affect investment decisions, because they do not account for seasonal and other effects that could correlate the residuals. For example, the same result would obtain if all funds hold more government securities in June or December, regardless of disclosure schedules, since a disproportionate number of funds disclose in June and December. To address this and other potential source of interdependence, I modify the test design in a way that controls more precisely for the incidence of disclosures.



The test design is borrowed from the abnormal-return literature: compare a fund's allocations around a disclosure date with an index of funds with different disclosure dates. As before, most of the complications follow from using weekly data. For this test it is convenient to number the MFR issues from 1 to 504 (the 11/24/87 issue being number 1), let  $I(m)$  be the number of the MFR issue closest to the end of month  $m$ , and let  $G_{f,i}$  be the government allocation of fund  $f$  in issue  $i$ .

Suppose fund  $f$  has a disclosure at the end of month  $m$ , so that  $G_{f,I(m)-12}\dots G_{f,I(m)+13}$  are  $f$ 's government allocations over the half-year surrounding a disclosure date, and  $G_{f,I(m)}$  is the allocation closest to the disclosure. The previous test is almost equivalent to comparing  $G_{f,I(m)}$  with the average of  $G_f$  over the 25 other weeks. For the new test I locate all funds which did *not* have disclosures at the end of  $m$ , let  $X_{m,i}$  be the average allocation of these funds in issue  $i$ , and then compare  $G_{f,I(m)} - X_{m,I(m)}$  with the average of  $G_f - X_m$  over the other 25 weeks. In the event-study terminology,  $X$  is the market index and  $G_{f,i} - X_{m,i}$  is  $f$ 's 'abnormal' government allocation on the date of issue  $i$ . The window dressing hypothesis is that the abnormal allocation is higher at disclosure time, which in this notation means  $i=I(m)$ .

An example illustrates this point. Figure 1 plots three variables from the six months surrounding 6/30/90. The line labeled *SCIT* is the government allocation of Scudder Cash Investment Trust, which had a disclosure on 6/30/90. This fund's government allocation was 2% on 7/3/90 and an average of 0.4% in the other weeks (*i.e.* 2% for five weeks and 0% for twenty weeks), for a difference of  $2.0-0.4=1.6\%$ . Meanwhile, the value-weighted index *VW* of funds that did not disclose on 6/30/90 (but which must have disclosed at some point in those six months) was 11.26% on 7/3/90 and 10.46% on average in the other weeks, for a difference of 0.80%, and the equal-weighted

index *EW* was 5.48% on 7/3/90 and 5.41% otherwise, for a difference of only 0.07%. So SCIT's bias toward holding governments at disclosure, after controlling for the market, is either  $1.60 - 0.80 = 0.80\%$  or  $1.60 - 0.07 = 1.53\%$ , depending on the choice of index.<sup>vii</sup> The test for window dressing is to calculate these statistics for every disclosure of every retail fund and determine whether they are significantly positive. As before, the test is repeated on the sample of institutional funds. The details of the test are in the appendix and the results are in Panel A of Table IV.

Retail funds' bias toward disclosing government holdings is positive and statistically significant relative to either index, and the point estimates of 0.20% and 0.27% are similar to the estimate from the previous test. Figure 2 plots these results in event-study fashion: the value for week  $i$  is the average of  $G_{f,l(m)+i} - X_{m,l(m)+i}$  across the 3783 disclosures with the indicated index. Some or all of the funds' preference for government securities during portfolio disclosures evidently relates directly to their fiscal years. And again, the test does not find a significant pattern when repeated on institutional funds. But before concluding that investment decisions interact with disclosure requirements, it is worth noting that funds have other responsibilities keyed to their fiscal years.

The IRS and the SEC both impose constraints on portfolio weights. The SEC constraints bind throughout the year, but some of the IRS constraints apply only to certain points in the fiscal year. In particular, a fund must obey the diversification standards of IRC 851(b)(4)(A) which do not apply to holdings of government securities and which apply *only* at the close of each quarter of the taxable year. Among other things, it restricts the amount the fund can have invested with any one issuer. The semi-annual disclosure dates analyzed above are two of the four dates per year when this standard applies, so the results above are consistent with a strategy of temporary portfolio adjustments not to

influence outsiders but to satisfy the IRS.

The IRS regulation seems potentially important to the results, but there are at least two reasons why it probably is not. First, the SEC has a substantially similar diversification constraint (17 CFR § 270.2a-7(c)(4)(i)) which is always binding, so the marginal effect of the IRS constraint at quarter-ends may be very small. And second, when the tests of Panel A of Table IV are modified so that the control sample is the two Tuesdays closest to the adjacent quarter-ends (*i.e.* dates when the IRS regulation applies but portfolios are not disclosed), rather than the 25 adjacent Tuesdays, the test statistic is still significant for the retail funds but not the institutional funds, and point estimates are similar. These results are reported in Panel B of Table IV.

#### **IV. Summary and Conclusion**

The data for this paper are fiscal year-ends and weekly portfolio statistics of taxable money funds. The interesting feature of this data, and the inspiration for the paper, is that the weekly statistics on allocations to government securities are uneconomical for retail investors to observe. This allows a direct test of the window dressing hypothesis of Haugen and Lakonishok (1988) and others on retail money funds, because we can compare the semiannual portfolios that the clientele can observe with those that they can't, and because government issues are generally regarded as less likely to default. Retail funds in the database which allocate between government and non-government instruments can misrepresent their default risk to their clientele by holding more in governments at disclosure than before or after. Several tests in the event-study tradition confirm that portfolio weights do in fact follow this pattern. The point estimates of the effect are modest, amounting to about \$1 billion. It is worth noting, however, that

the funds studied here *chose* to reveal their weekly portfolio statistics to MFR subscribers. This sample is biased toward funds which don't mind increased disclosure, which would intuitively be those that window dress relatively less.

Much of the current finance literature investigates whether agency problems distort the investment decisions of financial intermediaries. The evidence presented here indicates that money fund managers do not manage their claimholders' money as they would manage their own. That is, a private individual managing his own money has no disclosure events to interact with his investment decisions. This does not prove that consumers want their intermediaries to behave differently, or that fund managers incur extra transactions costs. What it shows is that the strategy known as window dressing is a real and measurable element of delegated money management.



### Appendix: Details of Test Design for Table IV

*Panel A:* For a given month  $m$  between February 1988 and March 1997 I identify  $I(m)$  and all money funds in the MFR database which for every issue from  $I(m)-12$  to  $I(m)+13$  have government allocation and assets-under-management data and are unconstrained retail funds. I divide these funds into two groups – those with fiscal quarters II or IV ending in month  $m$ , and all other funds. From the second group I create two indices  $EW_{m,i}$  and  $VW_{m,i}$ , where  $EW_{m,i}$  is the equal-weighted average government allocation in issue  $i$  and  $VW_{m,i}$  is weighted by assets under management reported in issue  $i$ ,  $i \in \{I(m)-12, \dots, I(m)+13\}$ . For a given fund  $f$  in the first group I calculate two statistics  $Z_{EW}$  and  $Z_{VW}$  as follows:

$$Z_{EW} = (G_{f,I(m)} - EW_{m,I(m)}) - \frac{1}{25} \sum_{i \neq I(m)} (G_{f,i} - EW_{m,i})$$

$$Z_{VW} = (G_{f,I(m)} - VW_{m,I(m)}) - \frac{1}{25} \sum_{i \neq I(m)} (G_{f,i} - VW_{m,i})$$

For every month I repeat this procedure for all funds in the first group, which yields 3783 observations of  $Z_{EW}$  and of  $Z_{VW}$ . I then run the same test on institutional funds.

*Panel B:* Same methodology, except now

and

$$Z_{EW} = (G_{f,I(m)} - EW_{m,I(m)}) - \frac{1}{2} ((G_{f,I(m-3)} - EW_{m,I(m-3)}) + (G_{f,I(m+3)} - EW_{m,I(m+3)}))$$

$$Z_{VW} = (G_{f,I(m)} - VW_{m,I(m)}) - \frac{1}{2} ((G_{f,I(m-3)} - VW_{m,I(m-3)}) + (G_{f,I(m+3)} - VW_{m,I(m+3)}))$$

which yields 3765 observations. Again, the test is repeated on institutional funds.

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**Table I**  
Fiscal year-ends of funds reporting to MFR as of 7/15/97  
Taxable unconstrained money funds

<i>end of:</i>	<i>Retail</i>		<i>Institutional</i>	
	<i>#</i>	<i>assets (\$B)</i>	<i>#</i>	<i>assets (\$B)</i>
January	4	0.881	20	13.539
February	11	9.430	6	6.529
March	24	58.665	17	20.117
April	8	11.376	12	14.104
May	10	11.835	6	7.717
June	31	44.976	6	2.387
July	16	41.367	8	10.434
August	24	23.581	14	24.736
September	22	13.916	12	27.233
October	39	31.080	11	7.488
November	15	61.820	10	9.978
December	76	109.496	33	28.430
<i>total</i>	280	418.424	155	172.691

**Table II**  
Summary Statistics of Allocations to Government Securities  
*In Percent*

For the 452 funds that are unconstrained retail funds in at least one week, the weeks in which it is an unconstrained retail fund are identified, and then the mean, median, minimum, maximum, standard deviation and interquartile range of the allocations to government securities in those weeks are calculated. Finally, the average of each statistic across funds is calculated and reported below. This procedure is repeated for the 220 funds that are unconstrained institutional funds in at least one week.

<i>Statistic</i>	<i>Retail Funds</i>	<i>Institutional Funds</i>
Mean	8.62	8.69
Median	7.24	7.78
Minimum	2.02	3.39
Maximum	27.90	22.41
Standard Deviation	6.12	4.80
Interquartile Range	7.60	6.75
Number of Funds	452	220



**Table III**  
**Disclosed minus Undisclosed Government Allocations**  
**Taxable Unconstrained Money Funds, 1988-1996**

*In Percent*

For every year  $y$  from 1988 to 1996, and for every unconstrained retail fund  $f$  that reported portfolio statistics for each Tuesday of  $y$ , the average allocation to government-issue instruments was calculated first over the two Tuesdays closest to  $f$ 's semiannual disclosures, the disclosed portfolios, and then over the remaining Tuesdays of  $y$ , the undisclosed portfolios. This yielded 1802 sets of two averages. The table reports the means of these two averages across the observations, the mean difference between disclosed and undisclosed averages, and the one-tailed t-statistic for the null hypothesis that the true mean is not positive.

<u>Portfolio Type:</u>	<u>Govt. Allocation</u>	<u># observations</u>	<u>t-statistic</u>
<i>Panel A: Retail Funds</i>			
Disclosed	7.974	1802	
Undisclosed	7.678	1802	
Difference	0.296	1802	3.75**
<i>Panel B: Institutional Funds</i>			
Disclosed	7.229	523	
Undisclosed	7.096	523	
Difference	0.134	523	0.87

\*\* - Null hypothesis rejected at the 1% level

**Table IV**  
Interaction between fiscal years and government allocations  
Taxable Unconstrained Money Funds, 1988-1997  
*In Percent*

**Panel A:** For a fund  $f$  whose second or fourth fiscal quarter-end is proximate to the Tuesday of week  $w$ ,  $G_{f,w-12} \dots G_{f,w+13}$  are its allocations to government securities on the 26 Tuesdays around  $w$  and  $X_{w-12} \dots X_{w+13}$  are an index of government allocations of all funds whose second or fourth fiscal quarter-ends are *not* proximate to  $w$ . *Index weighting* indicates whether the index is equal-weighted or weighted by assets under management, *mean* is the average of  $(G_{f,w} - X_w) - (1/25) \sum_{i=0}^{24} (G_{f,w+i} - X_{w+i})$  across quarters and funds, *t-statistic* is the one-tailed t-statistic for the null hypothesis that the true mean is not positive, and  $N(obs)$  is the number of observations included in the statistics. **Panel B:** Same methodology as Panel A, except that the number subtracted from  $G_{f,w} - X_w$  is not the average over the 25 adjacent weeks, but rather the average over the Tuesdays closest to the 2 adjacent quarter ends.

*Panel A: control is the 25 adjacent weeks*

<u>Index weighting</u>	<u>mean</u>	<u>t-statistic</u>	<u>N(obs)</u>
<i>Retail Funds:</i>			
Equal	0.1982	3.58**	3783
Value	0.2662	4.77**	3783
<i>Institutional Funds:</i>			
Equal	0.1346	1.38	1146
Value	0.1137	1.17	1146

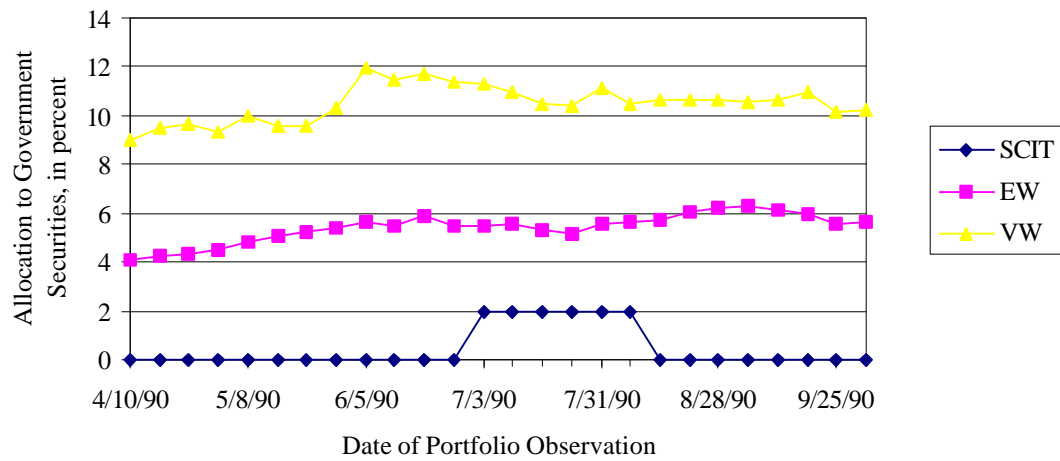
*Panel B: control is the 2 adjacent quarter-ends*

<u>Index weighting</u>	<u>mean</u>	<u>t-statistic</u>	<u>N(obs)</u>
<i>Retail Funds:</i>			
Equal	0.3146	3.80**	3765
Value	0.3306	3.95**	3765
<i>Institutional Funds:</i>			
Equal	0.0746	0.56	1136
Value	0.0536	0.40	1136

\*\* - Null hypothesis rejected at the 1% level

**Figure 1**

Scudder CIT around 6/30/90 Disclosure



**Figure 2**

Abnormal Government Investment Relative to Disclosure

